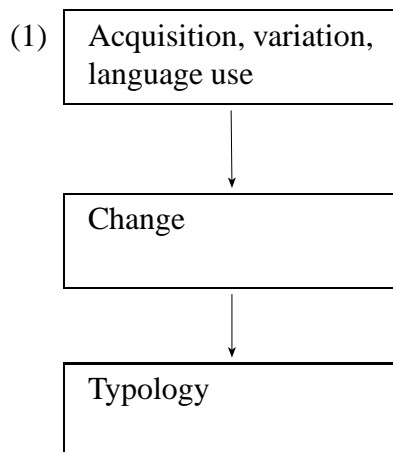


# The Amphichronic Program vs. Evolutionary Phonology

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## 1 Synchronic and historical explanation

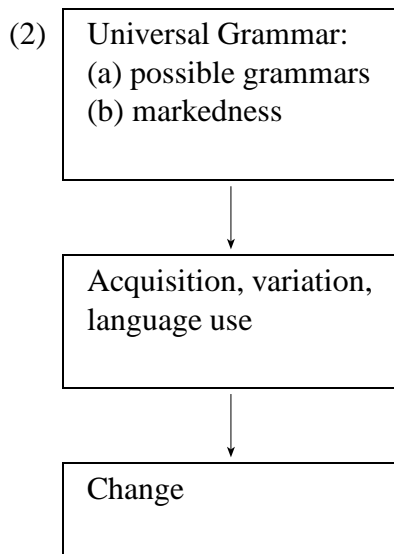
**Evolutionary Phonology.** Evolutionary Phonology seeks to derive typological generalizations from recurrent patterns of language change, themselves assumed to be rooted in perception, production, and acquisition. The goal is to eliminate UG by providing diachronic explanations for the cross-linguistic evidence that has been used to motivate it. (2) shows a schema of this program, where the arrows can be read as “explains” and/or “constrains”.<sup>1</sup>



Along with other historicist approaches, Evolutionary Phonology turns the traditional structuralist/generative view of the relation between synchronic and historical linguistics on its head. This assumes that change is constrained and explained by principles of grammar, so that diachronic change becomes evidence that can help to confirm or falsify those principles.

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<sup>1</sup>Thanks to Arto Anttila, Lev Blumenfeld, Andrew Garrett, and Alex Jaker for comments.



Blevins illustrates the difference between the two approaches with the example of neutralization of place and manner features in word-final and coda positions. Phonologists have postulated as a universal that marked features may be suppressed in such “weak” positions in favor of unmarked features, but not conversely. In OT, this putative universal is formally reflected by the existence of constraints that prohibit marked features in weak positions, and the absence of constraints that prohibit unmarked features in them. A markedness constraint may be defeated by a higher-ranked constraint (either by another markedness constraint, for instance one that enforces assimilation, or by faithfulness constraints). But it may not be reversed. The specific instantiation of this asymmetry that Blevins takes up is the devoicing of obstruents in coda and word-final portion. Blevins observes that under the evolutionary account,

“nothing excludes the inverse process of final obstruent voicing from the grammar of natural language. Rather, final obstruent voicing is predicted to be rare because there is no single documented natural process which would yield final voiced obstruents to the exclusion of voiceless obstruents, and there are few combined natural developments which would yield regular final obstruent [voicing P.K.] patterns.”

Of course the claim cannot simply be that final voicing rules are rare because final voicing as a single sound change is “undocumented”. The Evolutionary program is more interesting than that. It aims to explain *why* they are undocumented. In other words, Evolutionary Phonology seeks to ground typology in a substantive theory of sound change. Such a theory would provide, in the case at hand, a principled reason why no single sound change can produce final voicing.<sup>2</sup>

Does Evolutionary Phonology deliver on this front? Not quite yet, I think. Blevins suggests that one source of final devoicing is CHANGE — the reinterpretation of ambiguous phonetic signals — and that cues for laryngeal features in codas can be “absent, imperceptible, or difficult to perceive”. The ambiguity sets the stage for reinterpretations, but something else must explain the directionality of the change. In the absence of a cue for laryngeal features, the listener/learner interprets the neutralized signal as voicelessness. Why does she not instead guess that ambiguous signals are intended as voiced, which would produce final voicing? This is a special case of the

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<sup>2</sup>Moreover, combinations of natural developments that could lead to synchronic final voicing should be rare and there should also be a good reason for that.

directionality problem faced by reanalysis theories of every stripe. Theorists of analogical change have provided UG principles (such as markedness asymmetries of categories) to account for the directionality of reanalysis (Kuryłowicz, Wurzel, Drescher and Lahiri, to name a few). But the point of Evolutionary Phonology is to explain away UG principles, so it must find a different way of predicting the direction of reanalysis.

Because Evolutionary Phonology puts the explanatory burden on sound change, it must restrict sound change more severely than approaches which assume a richer UG. Models of the latter type can afford fairly unconstrained mechanisms of sound change because the results of sound change must pass through the filter of UG whenever they are acquired by a learner. Unnatural configurations resulting from sound change (the analog of “hopeful monsters” in evolutionary biology) would not be viable and they would fail to be reproduced in normal language transmission. Although this is not the only conceivable division of labor, it is attractive for several reasons. One reason is that it suggests a solution to the problem of how there can be implicational universals when both the antecedent and the consequent of the implication can change independently: the implicational connection is maintained because language remains under the control of UG. Evolutionary Phonology must somehow relocate those implicational connections in the process of change itself, but it is not at all clear how this can be done. For this theory it is problematic how there can be any implicational universals at all.

**Amphichronic linguistics.** I concur with Blevins that it is important to pursue the program of Evolutionary Phonology, and historical explanation in linguistics generally. She is right to stress that many putative universals are really byproducts of language change. For example, as Andrew Spencer and Alexis Manaster Ramer have pointed out, the reason why the process /k/ → [tʃ] before front vowels or glides is so widespread in synchronic grammars is not necessarily because it is natural, but because it is the end of a chain of natural processes set in motion by the palatalization of *k* in that environment: [k] > [c] > [tʃ] < [tʃ], as is well-known. In such a case, the presumption is that no synchronic UG-based account is required.

However, I also think that there are many cases where the causality goes in the other direction. Phonology cannot be “stripped of nearly all universal components” (Blevins 2004: 251). Linguistics must consequently work with both modes of explanation. This means pursuing both programs in tandem with a view to sorting out true universals from typological generalizations that are byproducts of tendencies of change (“accidental” universals, quasi-universals). This is what I argued in an article to which Blevins in part responds in hers.<sup>3</sup> As a contribution to this agenda I suggested a set of criteria for determining the division of labor.

- (3)
  - Universals should have no exceptions; typological generalizations are in principle *tendencies*. Whatever change can create, it can also destroy: therefore a structural feature due to recurrent patterns of change, however frequent, is intrinsically vulnerable. Even if it happens to be “accidentally” exceptionless, is still a tendency, for, as Blevins rightly remarks, “non-existence of particular sound patterns should not be interpreted as primary evidence of their impossibility”.
  - Universals should constrain any kind of change, and they should define “attractors” on which a variety of different diachronic paths converge. Typological generalizations should be associated with the specific diachronic paths that explain them — ideally unique paths, since convergence of multiple paths would itself require an explanation.

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<sup>3</sup>See Bermúdez-Otero 2005 and de Lacy & Kingston MS for a similar perspective.

- Another hallmark of a universal should be its spontaneous emergence within grammars, for example by non-exemplar-driven analogical processes such as grammaticalization (“Emergence of the Unmarked” effects in the OT sense).
- Knowledge of universals should be revealed in the way grammar unfolds in first language acquisition. Learners may construct grammars that violate typological generalizations, but they cannot, by definition, construct grammars that violate UG.
- Universals are rules or constraints that are part of every grammar and interact with each other dynamically in grammars. Typological generalizations are descriptive generalizations *about* grammars and stand outside of them. They do not necessarily correspond to rules or constraints *within* the systems that they are true of.

This much is just a program, not a theory, because it does not by itself tell us where the line between universals and typological generalizations will run. But it is a program with teeth. The criteria should converge, the universals should be grounded, and the typological generalizations should be consistent with, and indeed derivable from, the nature of linguistic change.

Blevins calls my approach “innatist”, but this is incorrect for two reasons. First, while the criteria I propose serve to distinguish intrinsic properties of language (“universals”) from historically contingent ones (“typological generalizations”), they do not and cannot tell us whether a putative universal, in this sense, is innate, grounded in language use, or both (it is good for us to be predisposed to learn the kinds of languages that are good for us — an instance of the so-called Baldwin effect). Secondly, I make no prior commitments to an innate faculty of language. I happen to find some of the arguments for it quite persuasive, but the program can just as well be pursued by those who do not, and indeed it may well turn out to undermine innatist assumptions.

A further point of caution is in order here: unlike typological generalizations, universals interact dynamically within a linguistic system. Hence “exceptionless” does not mean “inviolable”. In the spirit of OT, I assume that even true universal constraints may be violated, but only when a more highly ranked constraint forces it. For example: the constraint “codas are unvoiced” may be outranked by a constraint that requires voicing assimilation, or by a constraint that requires voicing to be realized.

For want of a better term, I’ll refer to this program as AMPHICHRONIC LINGUISTICS. Amphichronic linguistics will have to do battle on several fronts. On one side are the vigorous survivals and revivals of 19th century historicism, in the form of doctrines that reject the validity of synchronic explanation or assign diachronic explanation some kind of epistemological priority, and approaches that divide phonology from the rest of language and privilege physical accounts of the former and psychological accounts of the latter. On the other side, amphichronic linguistics encounters the 20th century superstitions that historical explanations are unprincipled and/or uninteresting, that synchronic explanation is the only kind there is (Lightfoot 2000), that even countenancing the possibility of competing historical and synchronic explanations is a “category mistake” (Gill, Harlow, and Tsoulas 2004), and that providing a historical explanation for a generalization amounts to reducing it to an “accident”. Somewhere on the side is PANCHRONIC LINGUISTICS, a typology-oriented processual approach whose founding gesture is to sweep aside the very issues that ambichronic linguistics places at the theoretical center of inquiry.

The same questions arise in other social and cultural sciences, but linguistics perhaps offers unique opportunities to resolve them empirically.

As an illustration of this program I considered the putative phonological universal that marked feature values are suppressed in “weak” prosodic positions. In the OT framework this universal can be formalized in several ways. The right way to do it in my opinion is that constraints can single out marked feature values (but not unmarked feature values). From these, with certain additional assumptions, we can build a system of constraints that asymmetrically prohibit marked feature values in weak positions. In processual terms, it predicts the existence of coda devoicing (coda depalatalization, debuccalization, deaspiration, etc.), and excludes coda voicing (coda palatalization, buccalization, aspiration, etc.). It does *not* exclude, for example, languages which contrast voiceless aspirates and voiced unaspirated stops everywhere, including in codas: these are straightforwardly predicted by constraints on contrast that outrank markedness constraints that enforce neutralization (Dispersion Theory, Flemming 2004, 2006).

We can then consider the question from the empirical side. For example, is coda neutralization, or more specifically coda devoicing, an intrinsic constraint on language (part of UG), or a typological generalization which is a consequence of the way sound change works? Blevins claimed that coda devoicing is just rare, and that the reason for its rarity is that it is unlikely to arise by sound change. Her theory thus predicts *actual* instances of synchronic final voicing, not merely *possible* ones. If no such processes are found, the evolutionary program is in trouble, because — as Blevins recognizes — they can readily arise by various combinations of sound changes. In fact, the number of potential scenarios that *could* produce a final voicing process is very large. Here are five of them.

(4) *Scenario 1: chain shift resulting in markedness reversal*

Stage 1: tatta tata tat (\*tatt) (gemination contrast)  
 Stage 2: tata tada tad (\*tat) (lenition)

- Result at stage 2: new voicing contrast, word-final phonological voicing.

*Scenario 2: lenition plus apocope*

Stage 1: takta tada (\*tata, \*data, \*tat, \*dat) (allophonic V\_\_V voicing, no final -C)  
 Stage 2: takta tad (\*tat, \*dat, \*dad, \*dat) (apocope, unless final \*-CC would result)

- Result at stage 2: allophonic voicing of word-final stops.

*Scenario 3: lenition plus deletion*

Stage 1: tat tad dat dad (voicing contrast)  
 Stage 2: tad tað dad dað (coda lenition)  
 Stage 3: tad ta dad da (loss of weak fricatives)

- Result at stage 3: only voiced obstruents occur in codas.

*Scenario 4: assimilation plus deletion*

Stage 1: tata tanta (no voicing contrast, only nasal codas)  
 Stage 2: tata tanda (allophonic voicing after nasals)  
 Stage 3: tata tand (apocope after heavy syllables)  
 Stage 4: tata tad (loss of nasals before stops)

Stage 2 is like Japanese. At Stage 3, final vowels are lost after heavy syllables, as in Old English. Finally, nasals are lost before voiced stops, as in Modern Greek.

- Result at stage 4: word-final allophonic voicing.

*Scenario 5: sound change plus analogy*

- Stage 1: saz atasa saz dasa sas tasa (final voicing assimilation)
- Stage 2: saz tasa saz dasa sas tasa (aphaeresis)
- Stage 3: saz tasa saz dasa saz tasa (analogical generalization of voicing)

At Stage 1, final obstruents undergo voicing assimilation. At Stage 2 voicing assimilation becomes opaque because initial vowels that trigger it are lost. Then the voiced obstruent is analogically generalized to all environments.

- Result at stage 3: word-final voicing.

In spite of the fact that such chains of well-known kinds of historical changes could easily produce synchronic coda voicing and final voicing, it seems, contra Blevins, that these processes are not merely rare but nonexistent in actual languages. Their absence tells against the evolutionary program, and shows that some constraint on language design (whether structural or functional) must be at work.

## 2 Are there synchronic final voicing processes?

Blevins presents five phonological systems that have an alternation which she argues should or at least can be analyzed as final voicing. I propose to show that Blevins' evidence falls short of establishing the existence of synchronic final voicing processes, and, more constructively, that there is in each case an analysis that does *not* posit final voicing and is superior for independent reasons. Somewhat surprisingly, in several cases this preferred analysis turns out to incorporate a final *devoicing* process, the exact opposite of what Blevins posits.

**Somali has no final voicing.** Somali has two sets of oral stops, standardly written /t k/ and /b d g/, and conventionally referred to as voiceless and voiced (Saeed 1999). /t k/ are always aspirated and voiceless, /b d g/ are always unaspirated and voiced or voiceless depending on the context. Phonetic studies emphasize that the two series differ saliently in tenseness and aspiration, and phonological analyses consider this a tense/lax or fortis/lenis contrast (Armstrong 1964, Pia 1965: 36), because, in Pia's words, "only those features seem truly distinctive, while others, such as voicing, were inconsistent in their occurrence." Blevins herself admits that "it would be accurate to treat /t k/ as aspirated in contrast to the unaspirated /b d g/".

The tense aspirated voiceless series appears only in initial and medial position. Word-finally it merges into the lax series. The lax stops have three pronunciations (Edmondson, Esling, and Harris 2004):<sup>4</sup> *Word-initially*, they are partly voiced or completely voiceless stops [b̥-], [d̥-], [g̥-], (or [b̥̆-

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<sup>4</sup>The retroflex implosive *dh*, initially [d̥-], is medially and finally preceded by a flap [-ɾ̥t̥(-)], word-finally devoiced [-ɾ̥t̥], e.g. *gabadh* [g̥ā β̥áɾ̥t̥] 'girl'. The epiglottal stop *c* is initially and medially [ʔ̥], finally [-ʔ], e.g. *sac* ['sāʔ] 'cow'.

] etc.).<sup>5</sup> *Medially*, they are voiced fricatives [-β-], [-ð-], [-ɣ-].<sup>6</sup> *Word-finally*, they are voiceless glottalized unreleased stops [-ʔp̚], [-ʔt̚], [-ʔk̚], except in “careful, overly correct speech”, where they are voiced stops with a voiced schwa offglide [-b<sup>ə</sup>], [-d<sup>ə</sup>], [-g<sup>ə</sup>]:<sup>7</sup>

- (5) *ilig* [ʔil̥iʔk̚] ‘tooth’, *tog* [ʔʰɔʔk̚] ‘tie up’, *bid* [ʔb̥iʔt̚] ‘fly up’ (dust), *cad* [ʔʦáʔt̚] ‘white’, *sacab* [ʔsáʔʦáʔp̚] ‘palm of the hand’, *sagaal* [sàʔyáál] ‘nine’ (Edmondson, Esling, and Harris 2004)

Taking /b d g/ as voiced unaspirated stops and /t k/ as voiceless aspirates, we require two weakening rules.

- (6) a. FINAL NEUTRALIZATION: Final stops are unvoiced, unaspirated, and unreleased.  
b. LENITION: Voiced stops are spirantized intervocalically.

(7) Input (after stress and vowel changes)	ʔilk <sup>h</sup> ó	ʔílik <sup>h</sup>	ʔedgó	ʔédeg
(6a) FINAL NEUTRALIZATION	—	ʔílik̚	—	ʔédek̚
(6b) LENITION	[ʔilk <sup>h</sup> ó]	[ʔílik̚]	[ʔedgó]	[ʔéðek̚]

Additional rules of phonetic implementation introduce the phonetic variation: partial voicing of stops in initial position, and glottalization of stops in final position, or in extra careful speech, a final schwa offglide with voicing (but no lenition).

Blevins argues that the two stop series differ phonologically in voicing: “... the fact that allophones of /b d g/ are voiced in all positions with the exception of final position when they are glottalized and unreleased, suggests that voicing is a phonological feature of the /b d g/ series.” (For a structuralist such as Pia, the phonetically predictable variation between voiced and unvoiced allophones of /b d g/ suggested precisely the opposite — that voicing is *not* a phonological feature of them.) If /b d g/ are phonologically voiced, they must undergo final devoicing. But Blevins claims that /t k/ also undergo final *voicing*: “the voiceless glottalized allophones are variants of phonologically voiced segments. Once this is granted, the alternations [...] must be interpreted as instances of word-final voicing.” I do not quite understand this argument. It apparently depends on a separation of phonological and phonetic features for which no argument is given. By the same reasoning as above, aspiration, glottalization, continuancy, and release should also be phonological features. But Blevins evidently means that *only* voicing phonologically distinguishes the two series, which implies the rules in (8) with the derivations in (9):

- (8) a. FINAL VOICING: Final stops are voiced.  
b. ASPIRATION: Voiceless stops are aspirated.  
c. FINAL NEUTRALIZATION: Final stops are unvoiced, unaspirated, unreleased, and glottalized.

<sup>5</sup>“Weak unaspirated” *p t k*, partially voiced or idiolectally completely unvoiced (Armstrong 1964). “The voicing may be present throughout or only at the end of the phone” (Pia 1964 :39).

<sup>6</sup>In line with most descriptions, I’ll take “medially” to mean “intervocalically”. According to Pia 1964, spirantization occurs between any voiced sounds.

<sup>7</sup>“Voiceless (or with slight voicing) and with no release” (Armstrong 1964: 4), voiceless stops (Pia 1964 :39), Lamberti (1988: 32). According to Dubnova (1990: 18) final /b/ and /g/ are fully voiceless, and /d/ can be either fully voiceless or partly voiceless.

d. LENITION: Voiced stops are spirantized intervocalically.

(9) Input (after vowel changes)	ʔilkó	ʔílik	ʔedgó	ʔédeg
(8a) FINAL VOICING	—	ʔílig	—	—
(8b) ASPIRATION	ʔilk <sup>h</sup> ó	—	—	—
(8c) FINAL NEUTRALIZATION	—	ʔílik <sup>ʔ</sup>	—	ʔédek <sup>ʔ</sup>
(8d) LENITION	[ʔilk <sup>h</sup> ó]	[ʔílik <sup>ʔ</sup> ]	[ʔedgó]	[ʔéðek <sup>ʔ</sup> ]

When final schwa offglide is pronounced after /b d g/ “in overly careful speech”, they are voiced, but not spirantized like phonemically intervocalic lax stops. So a separate phonetic implementation rule is required, as in the previous analysis.

(8), (9) is inferior to (6), (7) for several reasons. Final voicing must simply be undone at the phonetic level, in “Duke of York” derivations such as /ilik/ → ilig → [ilik]. This complicates the rules, and requires undesirable extrinsic rule ordering. In fact, the final voicing rule is unnecessary because the derivations without it give the same result:

(10) Input (after vowel changes)	ʔilkó	ʔílik	ʔedgó	ʔédeg
(8b) ASPIRATION	ʔilk <sup>h</sup> ó	—	—	—
(8c) FINAL NEUTRALIZATION	—	ʔílik <sup>ʔ</sup>	—	ʔédek <sup>ʔ</sup>
(8d) LENITION	[ʔilk <sup>h</sup> ó]	[ʔílik <sup>ʔ</sup> ]	[ʔedgó]	[ʔéðek <sup>ʔ</sup> ]

If aspiration and voicing are both considered to be phonological features (as I think follows from Blevins’ own argument if pursued consistently), then step (8b) can be omitted as well, and we are back to the even simpler analysis I proposed in (6).

I conclude that Somali has no final voicing process.

**Welsh has no final voicing.** Welsh has two sets of oral stops, standardly written /p t k/ and /b d g/, conventionally referred to as voiceless and voiced. Phonologically and phonetically, /p, t, k/ are aspirated and /b, d, g/ are unaspirated (Jones 1984: 41 ff., Ball 1984: 15, Isaac 2004<sup>8</sup>). According to Jones, they are normally *voiceless* in word-initial and word-final position, and usually medially as well, e.g. [b̥is] ‘finger’, [ʃið̥] ‘day’, [kiɡ̥] ‘meat’, [seβ̥on] ‘soap’, [blod̥ɛ] ‘flowers’. They may be partially voiced in all these positions, and occasionally fully voiced in medial position. In the native vocabulary of Welsh the aspirates are deaspirated word-finally, merging with the unaspirated series. In short, the Welsh stop system is similar to that of Somali, except that there is no medial spirantization and voicing is less frequent.

Blevins does not stipulate synchronic final voicing for Welsh. Rather, she says that, were it not for “accidents of history”, it *would* have final voicing. That is, she transposes the Welsh aspiration contrast into a voicing contrast, for that hypothetical stage of the language anyway. I cannot agree with Blevins’ assertion that “what is relevant in this case, is not the correct synchronic analysis, but the set of facts themselves”. On the contrary, it is precisely on the correct synchronic analysis that the argument turns. The issue is whether actual synchronic rules of final voicing exist in real languages. No such rule is documentable for any present or past variety of Welsh.

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<sup>8</sup>“The so-called ‘voiced stops’ of Welsh are not distinctively voiced at all, i.e. they are not /d/ as opposed to /t/. Rather, they are distinctively non-aspirated /t/ as opposed to the aspirates /th/. This phonological /t/ has voiceless and voiced allophones [t] ~ [d].”



**Tundra Nenets has no coda voicing.** Blevins' claim that Tundra Nenets has synchronic coda voicing is based on Salminen's statement (1998: 524) that codas allow /b/, but not /p t k d/ or any other oral stop, and on the forms *ɲob* 'one' (Nom.Sg.), *ɲobta* 'his/her one', *ɲopoy* 'the one' (moderative) (Salminen 1997:71).

For similar reasons as in Somali, Tundra Nenets /p t k/ vs. /b d/ is treated as contrast of tenseness, not of voicing (Lehtisalo 1956,<sup>9</sup> Mikola 1975: 23,<sup>10</sup> Hajdú 1988,<sup>11</sup> Janhunen 1986: 55-56, Salminen 1997, 1998). /p t k/ are tense stops, and markedly long in medial position, whereas /b/ and /d/ are lax, and articulated with varying kinds of lenition.<sup>12</sup>

In fact, *ɲob* and *ɲobta* are pronounced [ɲōbʔ], [ɲōbʔta], with a *voiceless* lax stop, which in prepausal position is followed by a glottal stop.<sup>13</sup> Word-internally, and word-finally in close contact with the following word, there is no glottal stop, but the stop is still always voiceless [b].<sup>14</sup>

The lax /b/ appears throughout the paradigm of this word, not only in coda position but also in onsets. In onset position, /b/ is realized as *partly voiced*: e.g. Acc.Pl. /ɲobɔ/ → [ɲōb̥ɔ], [-u] (Lehtisalo: ɲōB̥ʔù).<sup>15</sup> Since the lax stop is partly voiced in onsets and fully *voiceless* in codas, there is simply no question of any coda voicing here.

Blevins overlooks that Salminen cites forms in phonemic representation, and that his distributional generalizations are not about the phonetics, but about his fairly abstract phonemic level. At the *phonetic* level, it is simply not true that *b* is the only oral stop that occurs in codas. On the contrary, "almost all consonant and glide phonemes can occur in prepausal position at the surface" (Janhunen 1986: 109), including the tense voiceless stops. In word-final position, there is actually a four-way contrast:

<sup>9</sup>Lehtisalo's fine-grained transcriptions render the stops of the tense series as *-pp-* *-tt-* *-kk-* *-ʔis-* (where 'ʔ' marks a syllable boundary in the Finno-Ugric ("UPA") phonetic notation that he uses). They are never lax, voiced, or spirantized. The stops of the lax series have a more variable realization. Their invariant property are short duration and laxness. ("Extremely rare" gemination of the lax series has been recorded in a few "aberrant dialects", primarily after ə (Janhunen 1986: 55).) The invariant cues distinguishing the two series, then, are tenseness and length.

<sup>10</sup>"...da in dem jurakischen Konsonantensystem nicht der Gegensatz von stimmhaft — stimmlos zur Geltung kommt, sondern von gespannt — gelöst." Mikola also argues that the language's so-called "voiced" and "voiceless" glottal stops (phonetically distinguished by at least one speaker, Janhunen 1986) must differ in tenseness, for a voicing distinction in glottal stops is physically impossible. However, Janhunen's remarks indicate that the distinction may be a matter of nasal release.

<sup>11</sup>Hajdú states that /p t k/ are realized "mit gespannter Verschlussbildung" whereas /b d/ have "eine eigenartige Verschlussbildung mit einem Reibmoment, eine kraftlose, lockere Schlawffheit", and that "hier nicht der Gegensatz stimmhaft — stimmlos, sondern der Gegensatz tense — lax auftritt".

<sup>12</sup>Salminen uses the more non-committal "strong" and "weak". Lehtisalo shows the lax series variously as voiceless lenes [b̥ d̥ g̥ ʔ] (B D G DZ in his transcription), partly voiced [b̥ d̥ g̥ ʔ] (B̥ D̥ G̥ DZ), wholly voiced [b d g ʔ], weakly articulated [b̥, d̥] (*b, d*, "energielos", "vielleicht auch nicht ganz stimmhaft", p. CVII), and/or as voiceless lenis spirants [ɸ, θ] (Lehtisalo's B, D).

<sup>13</sup>In Lehtisalo's (1956: 38-39) UPA transcriptions *ɲōB̥*, *ɲōB̥ʔ*, the voiceless lax [b̥] is written *B*, and the glottal stop " is optionally followed by *ʔ*, which denotes a voiceless [h/ə] offglide ("einen stimmlosen unbestimmten Vokalhauch", Lehtisalo 1956: CVII). Salminen's citation forms systematically omit such final glottals because they are predictable from an automatic phonetic process, as he is careful to explain (1997: 31). The prepausal glottals are, however, marked in the standard orthography (e.g. Tereščenko 1965) and in the phonemic representations of Janhunen 1986.

<sup>14</sup>UPA *B*, e.g. *ɲōB̥ʔ* 'one' (Lat.Sg.), *ɲōB̥ t̥amn'e* 'one after the other' (Lehtisalo 1956: 38b).

<sup>15</sup>The word *ɲopoy* 'the one', which Blevins cites without further analysis, seemingly suggesting that coda *b* alternates with onset *p*, is a red herring. It is formed with the moderative suffix /-poy/, with degemination of /-pp-/ to *-p-* (Salminen 1997: 60). For the moderative suffix, compare *ɲarək̥k̥p̥p̥j̥ j̥əχp̥ə* 'a largish river', *j̥əχ̥əmb̥ʔi* 'a sort of river', 'a mid-sized river' (Lehtisalo 1956: 79a).

(11)		IPA	Lehtisalo	
A.	voiceless lax stop + ?	[-b̥ʔ]	[ŋōb̥ʔ]	ŋōB <sup>o</sup> , ŋōB <sup>o</sup> ʔ (38a) ‘one’
B.	voiced lax weak stop/spirant	[-b̥, -ϕ]	[jāb̥]	jāb̥ (101a) ‘luck’
C.	voiceless fortis stop	[-p]	[šēp]	šēp (440b) ‘pocket’
D.	voiced spirant/approximant	[-β]	[šɛβ]	šɛβ (406a) ‘eye’

Janhunen treats the prepausal glottal stop as phonemic and analyzes the word-final voiceless lax stops in series (A) as clusters, e.g. /-bʔ/. For Salminen, on the other hand, they are simple lax stops, and the series (B) stops are followed at the phonemic level by /-ə/, which is phonetically deleted but manifested in the voicing of the preceding consonant. In Salminen’s solution, the prepausal glottal stop is predictable, and he omits it from his phonemic representations. In word-initial position, only the tense series occurs, and Janhunen (1986) posits a single underlying stop series {p t k} at his “deep” morphophonological level. He derives the lax series by postvocalic lenition (not coda lenition!), e.g. Nom.Sg. {ŋop} → /ŋobʔ/ → [ŋōb̥ʔ], and the postvocalic /p t k/ (series C) from {ʔp ʔt ʔk}.<sup>16</sup> Final [-β] (series D) is /-w/ on both analyses.

To repeat: there is no coda voicing in Tundra Nenets. Blevins’ case rests on a misunderstanding of Salminen’s analysis. In fact, that analysis entails coda *devoicing*: at the phonemic level, prior to final ə-deletion, the lax stops must get specified as voiceless in codas and as fully or partly voiced in medial position.

**Italic had no final voicing.** Historical grammars posit a sound change of Indo-European *\*-t* to Proto-Italic *\*-d*. Forms in *-d* are attested in early Latin (e.g. *sied* ‘were’, *feced* ‘made’ in the 6th century Duenos inscription) and in Oscan.<sup>17</sup> Such a sound change does not by itself entail a *synchronic* final voicing rule for early Latin or Italic. Blevins does suggest that some stage of Italic *might* have had such a rule (“there is no evidence in this case against a general final obstruent voicing process”), but even this weak claim is dubious. Final *-d* contrasted with *-t* (from *\*-ti*), a synchronic rule “final *-t* is voiced” would not work, and there would have been just one morphological alternation (3.Sg. present *-t* vs. past *-d*), which could not have been perceived by learners as anything but morphologically conditioned allomorphy.

The traditionally assumed sound change is not consistent with Blevins’s claim that final voicing cannot be introduced directly as a single sound change. Therefore she proposes that *\*-t* > *-d* happened in two steps: intervocalic obstruent voicing followed by final vowel loss. While this is hardly a plausible scenario for Latin (what vowel loss could have been involved at the second stage?), perhaps there is some other plausible path that is consistent with her approach. I think, though, that there is no need to worry about what that might be, for there is a better historical account, on which there was no sound change *\*-t* > *\*-d* at all, either directly or in the two stages posited by Blevins.

Early Latin, and the other Italic languages, had a phonemic contrast between two series of voiced obstruents: the stops [b d g] inherited from Indo-European (as in Latin *edō* ‘eat’), and the fricatives [β δ γ] derived from the I.-E. voiced aspirate stops (as in Latin *medius* ‘middle’, from

<sup>16</sup>Salminen also gets postvocalic /-b/ from {-p} by postvocalic weakening. Such analyses posit two layers of opacity, but I think they could be implemented in Stratal OT if Janhunen’s morphophonemic, phonemic, and phonetic level are identified with Stratal OT’s stem, word, and postlexical level, respectively.

<sup>17</sup>The 3.Sg. *-t* of Classical Latin is assumed to be a later analogical generalization of the primary ending derived from *\*-ti*.

\**medhyos*).<sup>18</sup> By the time of classical Latin, the two series had fallen together into a single series of voiced stops.

When did this merger take place? A clue comes from the fourth voiced fricative [z], which was also eliminated, becoming [r] (“rhotacism”). The assumption that the four voiced fricatives were eliminated together by the sound change [β δ γ z] > [b d g r] can be justified by typological parallels, in particular the identical sound change in early Germanic. By this reasoning, the sound change can be dated, for Latin, to the 4th century B.C. by the appearance of rhotacism in written records.

But the Latin inscriptions where final \*-t is written -d are all earlier than that, from the 5th and 6th centuries. This tells us that these earliest records of Latin still represent the pre-merger system with /β δ γ z/, written *b d g s* respectively,<sup>19</sup> which means that the letter *d* in these texts stands for both [δ] and [d]. So, at this stage, the final -d in *sied*, *feced* could in principle be [-δ] or [-d].

Of these two possibilities, [-δ] is more likely, for two reasons. First, subsequent sound changes show that it was a phonetically weak sound: it was lost after long vowels in Roman Latin (e.g. 3.Sg. imperative -*tōd* > -*tō*), and it was lost in *all* contexts in other early dialects of Latin (e.g. Tivoli *dede CIL I<sup>2</sup> 47*) and in Faliscan (Baldi 1999: 172). The second reason for assuming [-δ] is that lenition of -t fits into a more general pattern by which *all* final obstruents and nasals were lenited in early Latin. We know this because inscriptions often omit final -s and -m, because they can be ignored in calculating syllable weight in poetry, and because later Latin writers mention the lenited pronunciation of these final consonants in older or rustic varieties of Latin. I conclude that the most likely interpretation of the sound change is that word-final \*-t became lenited, probably to a fricative or approximant [-δ].

To summarize: there probably never was a coda voicing sound change such as \*-t > -d, and in any case there was no synchronic /-t/ → [-d] rule in Italic.

**Lezgian has no final voicing.** Blevins’ case for final voicing in Lezgian cannot be faulted on the same grounds: it agrees with existing phonological and phonetic descriptions of a real language. Lezgian has four distinct series of stops: voiced, glottalized, and aspirated, and a fourth series which is realized as a long voiced stop in coda position and as a plain voiceless stop in onset position, and which occurs only *before the main stress*. Following Yu (2004), Blevins analyzes the voiceless onset alternant in this fourth series as basic, and posits voicing and lengthening in coda position.

In the alternative analysis presented in Kiparsky (2004) there is no final voicing process. The coda alternant, phonologically a voiced geminate, is taken as basic, and is degeminated and devoiced in onsets. This gives Lezgian a three-series stop system /D/ : /T’/ : /T<sup>h</sup>/ which is found in other Caucasian languages such as Kabardian and Adyghe and in native American languages such as Yana and Acoma; the restriction of the geminate series to pretonic position recalls other instances of pretonic gemination.

Blevins has three objections to my proposal. The first is that onset devoicing is unnatural because devoicing is weakening and onsets are strong positions. An evolutionary phonologist is

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<sup>18</sup>Whether via *fθx* by medial voicing/laxing, or directly by spirantization, with subsequent initial devoicing/fortition (Baldi 1999).

<sup>19</sup>The closely related languages seem to have had the same phonemic contrast between voiced stops and fricatives, as shown by Faliscan spellings like *carefo* ‘I will lack’ = Latin *carēbō*, with *\*bh* > β > *b*. My analysis obviously has implications for these other languages but this is not the place to go into them.

hardly in a position to fault synchronic analyses for insufficient naturalness; the corresponding claim in that framework would presumably have to be that onset devoicing cannot arise through sound change, which I doubt can be justified. But as a matter of fact, initial neutralization of medial voicing contrasts is not unheard of, e.g. Mordva (Zaicz 1998: 186) and Ewondo (Westbury and Keating 1986, who provide phonetic justification for initial devoicing); the best-known case is Lac Simon Algonquian, where initial devoicing appears to have been at least a sound change if not a synchronic process as well (Kaye 1979, Iverson 1983).

Secondly, Blevins says that if the /D/ series occurs geminated, then so should the voiceless /T'/ and /T<sup>h</sup>/ series, on the grounds that languages with underlying voiced geminate obstruents have voiceless geminate obstruents. Typological arguments based on putative implicational universals are precarious in a framework which has problems with implicational universals to begin with. But the generalization on which it is based appears to be false. As Blevins certainly knows, the standard phonemic analysis of Somali admits underlying voiced geminate obstruents, such as /bb/ /dd/ /gg/ (e.g. *oggol* 'to be willing', *xiddig* 'star'), but no voiceless geminate obstruents such as /pp/ /tt/ /kk/ (Armstrong 1964, Maddieson 1984: 314, Saeed 1999: 16).<sup>20</sup> I conjecture that in a /d/ : /t<sup>h</sup>/ system or in a three-way system /d/ : /t'/ : /t<sup>h</sup>/ such as that of Lezgian, /d/ is the unmarked series, voiced by dispersion (contrast maximization), and that if only one stop series geminates, it will be that unmarked series.

Third, Blevins says that my proposed degemination and devoicing rule must be extended to “pre-approximant codas” in words like *cekwre* /tsek<sup>w</sup>ré/. Here Blevins presupposes, without argument, that the *-k<sup>w</sup>*- in this word is a coda. Lezgian allows word-initial onset clusters of two and even three consonants (Haspelmath 1993, 46). The common strategy of onset maximization predicts the syllabification /tse.k<sup>w</sup>ré/, where the consonant in question is in an onset, which allows my rule to stand unmodified.

**Conclusion.** None of the five languages nominated by Blevins as candidates for synchronic final voicing provide a compelling case. For four of them, Blevins' arguments are based on reimagining voiceless stops as voiced, against the known phonetics, and/or on hypothetical stages which are unlikely to have ever existed. In the fifth case, there is at least as good an alternative on offer. So we come back to the original point: if no languages in fact have synchronic final voicing rules, even though such rules could easily come in “through the back door” by combinations of familiar kinds of sound changes, then there must be some intrinsic (UG) constraint that prevents them from arising. In Kiparsky 2004 this argument is complemented by evidence from the other five criteria mentioned above, all of which point to the same conclusion.

Of course, much further research is required. Even though the quest for synchronic final voicing processes has drawn a blank so far, it should be prosecuted vigorously, and extended systematically to other types of counter-natural coda neutralization, and indeed to the whole range of putatively impossible phonological processes whose existence evolutionary phonology predicts. Even though an unattested process may be possible, the generality of the hypotheses, and the availability of independent criteria from other domains than change, should eventually afford an empirical resolution of the question of phonological universals.

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<sup>20</sup>But see Ségéral and Scheer (2001) for an interesting argument that some voiceless singleton /t/ and /k/ phonemes are “virtual” geminates at a deeper level of analysis.

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